possibly have been made, it will be forwarded for criticism to the Committee (on Geometrical Teaching) of the British Association and to other mathematical authorities. The object, we further learn, is, if possible, to get the sanction of the British Association ; and this backing the opinion of the large number of mathematical teachers who now form the Association, will, it is hoped, lead the examining bodies of the country to act with perfect impartiality in considering the merits of those pupils who have been trained in accordance with the methods of the Syllabus as contrasted with the favourers of Euclid.

From the Report we gather that the principal work of the Association is expected to be completed in another two years ; it is not attempted to forecast what will be its subsequent work. Perhaps, as has, we believe, been suggested, it may become an Association for the Improvement of Mathematical Teaching.

As the publications of the Association are for private circulation, we cannot go into further detail; we may, however, say that it has done good work in having been the moving cause of five valuable Presidential Addresses.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

Influence of Pigments on the Photographic Image of the Spectrum

WHEN, some time since, Prof. H. Vogel announced the discovery that the addition of a pigment to a film of bromide of silver made it sensitive to light of the colour which that pigment gave it, though it had not been so previously, many—indeed I might say most—photographic chemists doubted the accuracy of his observations and the existence of any such law. His experiments were rehearsed by most of them, and the reports were, in almost every case, contradictory of his conclusions. There were powerful *à priori* reasons for doubting, amongst which the chief was, in my own opinion, that if a film coloured (say) red were sensitive to red light, it could not be developed under red light, but would fog, and would therefore be unworkable, which was not found to be the case. Another was, that the use of tinted films, well known for a long time, had only resulted in an universal retardation of all colours. It was, moreover, contrary to the known analogies of actinism that a purely mechanical admixture irrespective of any chemical quality should produce changes of so purely chemical a nature as those which are the basis of photographic action.

By the kindness of Mr. Lockyer I was enabled to experiment at his laboratory at South Kensington with the same plates (Col. Wortley's tinted films) that Prof. Vogel had based his discovery on, and, as I expected, found the results quite other than those the professor had announced. Although a protracted exposure (seventeen minutes) was given, and the more refrangible lines were quite buried by halation, no line was shown which did not appear in the ordinary wet collodion film.

That careful and excellent photographic chemist, Mr. Spiller, President of the London Photographic Society, Dr. Van Monckhoven, Mr. Carey Lea, and numerous others, amongst whom I am enabled, by his personal assurance, to name Dr. J. W. Draper, unquestionably the first living authority on spectrum photography, as well as his not less well-known son, Prof. Draper, have also followed Vogel in his experiments without obtaining any confirmation of his law.

Up to this time the only testimony confirmatory of his views offered is that of Becquerel, who, as the most marked instance of success, gives this—that chlorophyl (a green substance) gives great sensitiveness to *red* rays! That most indefatigable and precise experimentalist, Mr. Carey Lea (of Philadelphia, U. S. A.), in the course of a long series of experiments, unfortunately interrupted by his ill-health, showed that while coralline in a film did add slightly to the length of the spectrum image, other red pigments produced no effect whatever, and that salicine, which has no colour, produced more effect than coralline. But if chlorophyl, a green substance, is sensitive to red light, aniline green, so far as my own experiments go, produces no effect whatever except prolongation of the exposure necessary.

Now, without in the least disputing the prolongation of the spectrum photograph as claimed by Prof. Vogel, or depreciating the importance of his results, it seems to me that we are in a position to assume that he is entirely mistaken in the nature of the law he deduces, and that these results are due to purely chemical causes, in no wise dependent on colour, though in a few cases the colour may coincide with the chemical cause in such a way as to afford apparent confirmation of his hypothesis.

It must be remembered that Dr. Draper has long ago shown that all the rays have chemical activity, and that he has, without any such aid as Vogel has called in, produced complete photographic spectra ; and has also shown that different substances decompose under different rays. Becquerel's experience with chlorophyl gives a clue to the connection between these discoveries and Vogel's results, if collated with a series of phenomena resumed by Dr. Draper (from observations by Dr. Gardner) in the interesting papers by him on the "Distribution of Chemical Force in the Spectrum" * :—"In Dr. Gardner's paper there are also some interesting facts respecting the bleaching or decolorisation of chlorophyl by light. He used an ethereal solution of that substance :—'The first action of light is perceived in the mean red rays, and it attains a maximum incomparably greater at that point than elsewhere. The next part affected is the indigo, and accompanying it there is an action from + 10·5 to + 36·0 of the same scale (Herschel's), beginning abruptly in Fraunhofer's blue. So striking is this whole result, that some of my earlier spectra contained a perfectly neutral space from − 5·0 to + 20·5, in which the chlorophyl was in no way changed, whilst the solar picture in the red was sharp and of a dazzling white. The maximum in the indigo was also bleached, producing a linear spectrum as follows :—

in which the orange, yellow, and green rays are neutral. These, it will be remembered, are active in forming chlorophyl.' . . . I have quoted these results in detail, because they illustrate in a striking manner the law that *vegetable colours are destroyed by rays complementary to those that have produced them*, and furnish proof that rays of every refrangibility may be chemically active." (P. 7, "Researches in Actinic Chemistry.")

Dr. Draper goes on in this memoir to establish a second proposition to this effect : "That the ray effective in producing chemical or molecular changes in any special substance is determined by the absorptive property of that substance." This proposition, laid down in 1841, seems to me to contain the explanation of all the phenomena of chemical or molecular change in photographic films ; and if I might be permitted to offer an hypothesis supplementary to the proposition, serving, *if demonstrable*, as corollary to it, it would be that if two substances having different absorptive properties are simultaneously (or nearly so) subjected to the action of white light, in molecular contact the change in one of them may be communicated to the other mechanically. Thus, bromide of silver, which is not sensitive to the red ray, being placed in contact with chlorophyl, which *is* sensitive to that colour, the action of the red ray is communicated from the latter to the former substance, producing what may be designated as a sympathetic molecular effect. But in order that this may obtain, it is necessary that the auxiliary substance applied to influence the sensitive photographic film should be in itself sensitive to other rays than those which decompose the silver bromide. This would account for the effect of chlorophyl and perhaps for the original experiment which attracted the attention of Prof. Vogel, as the dry plates of Col. Wortley with which it was made contain salicine in their preservative as well as an aniline red in their substance, and Mr. Carey Lea has shown that salicine has the effect which Vogel claims for the colour.

If this is tenable, it follows that the object of our researches should be to discover those substances which have an independent susceptibility to actinic action, but for different rays than those which form the basis of the film experimented on. The results so far obtained in this direction, even those of Vogel himself, are, it seems to me, quite as capable of explanation by the hypothesis I have offered as by that of an arbitrary effect of colour; in confirmation of which we have only experiments (thus far made public) by Prof. Vogel himself.

It seems to me incredible that, if such a law existed, such

* "Researches in Actinic Chemistry, Memoir Second," &c. John William Draper, M.D., LL.D., New York.

careful and experienced investigators as the Drapers, Von Monkhoven, Spiller, Carey Lea, and others who have repeated Prof. Vogel's experiments, should utterly fail to obtain any confirmation of his hypotheses; and there is no solution in accordance with known facts and analogies of actinic action except to conclude with Dr. Draper that Prof. Vogel has made a mistake—he has attributed to one of two coincident qualities of certain substances effects which are due to the other.

Dr. Draper records experiments in which he secured a photograph of the entire spectrum on a daguerreotype plate, by availing himself of the singular reversing action of light on the impressed plate (pp. 2 and 3 of memoir), and allowing a diffused daylight to fall on the plate simultaneously with the spectrum image. "If," he says, "a spectrum be received on iodide of silver formed on the metallic tablet of the daguerreotype, and carefully screened from all access of extraneous light, both before and during the exposure, on developing with mercury vapour an impression is evolved in all the more refrangible regions.

"But if the metallic tablet during its exposure to the spectrum be also receiving diffused light of little intensity, as the light of day or of a lamp, it will be found, on developing, that the impression differs strikingly from the preceding. Every ray that the prism can transmit, from below the extreme red to beyond the extreme violet, has been active. The ultra-red heat lines $\alpha\ \beta\ \gamma$ are present."

The whole of this memoir is of the greatest interest to the spectroscopic photographer, not only as giving the result of all previous experiment in this field, but in clearly marking out what remains yet to do in it. The subsequent success of the younger Draper in obtaining a negative of the spectrum complete by the ordinary collodion process, through the aid of an analogous system of protection by mechanical means for the lines most readily impressed, proves that even with silver, and under any condition of process, we have the power of recording any spectroscopic phenomenon; but if experiment should prove that substances in themselves liable to decomposition by rays which do not attack the salts of silver are capable of communicating an impression by molecular contact *to* the silver, and inducing decomposition in it, it is evident that a complete combination may be arrived at which, without mechanical contrivances, will give us printing negatives of the spectrum throughout.

W. J. STILLMAN

Dr. A. B. Meyer and his Critics

NOT until now have I found leisure to look through the pages of NATURE for the years 1873 and 1874, and therefore it was not till now that I became aware of two letters in your correspondence (December 11, 1873, p. 102, vol. ix., and April 23, 1874, p. 482, vol. ix.), which concern me, and in answer to which I beg leave to say a few words.

The first is written by Mr. Wallace, and is about a wrong opinion which I had formed on this author's notion as to the relation of the inhabitants of the Arfak Mountains on New Guinea to the inhabitants of the coast. I am glad to see that Mr. Wallace and I agree in the conviction of the identity of those two groups of Papooas; but nevertheless I am anxious to show that my misunderstanding of Mr. Wallace's opinion was based upon an apparently clear expression in his valuable work on the "Malay Archipelago," which I took, as I believe, not in the restricted sense in which the author perhaps wished it to be understood. Mr. Wallace did not succeed in finding the passage in his work on which I had based my idea; but he just breaks off his quotation where the words begin to which I referred: "Their hair, though always more or less frizzly, was sometimes short and matted," &c.; so far Mr. Wallace cites his own words, but the sentence (page 310, 1st ed.) goes on, "instead of being long, loose, and woolly; *and this seemed to be a constitutional difference, not the effect of care and cultivation.*" These last words then led me to the opinion in question. In a paper in the *Mittheilungen der Anthropologischen Gesellschaft zu Wien* ("Anthropologische Mittheilungen über die Papúas von Neu Guinea; I. Ausserer physischer Habitus"), 1874, page 92, I quoted myself the *whole* passage, and dealt with the object more in particular. That it is still the general opinion that a difference exists between the Arfakis and the Papooas of the coast is proved, *e.g.*, by a notice of that paper in M. Broca's "Revue d'Anthropologie," vol. iii., 1874, page 729: "Notre voyageur n'admet pas non plus qu'il y ait entre les tribus du bord de la mer et celles des montagnes—les Arfakis—les différences constitutionelles *observés cependant par la plupart des voyageurs*," &c.

The other letter contains a protest of Signor D'Albertis against my having "led the public to believe that he had claimed for himself the honour of crossing New Guinea from one coast to the other." Signor D'Albertis cites my paper in NATURE, vol. ix. p. 77, where he states he has read an assertion of mine concerning this point. But I look in vain through my whole article to find one single word to the purpose, and therefore I do not understand what induced that intrepid co-operator to publish his protest. I only mentioned (page 79): "I need not say that this journey from one side of New Guinea to the other has never been made before, and I should hardly myself attribute any importance to the fact," &c.

A. B. MEYER

The Chesil Bank

THE letter of your correspondent, Col. Greenwood (vol. xi. p. 386), has only now been brought under my notice.

There is one fallacy contained in it which no one would detect more easily than Col. Greenwood, if he were but to visit the Portland end of the Chesil Bank. He would then see for himself that Portland Island *does not* act as a groin in accumulating the pebbles that form the beach.

The Chesil Bank extends from Portland to Bridport Harbour, where it is composed of small pebbles or gravel of the average size of horse-beans. It is there a true beach of considerable breadth and depth, and does not merge into sand until it arrives at a point beyond the mouth of the harbour. Following it towards Portland, it runs along under the cliffs by Burton, Swyre, &c., to Abbotsbury, where it assumes its distinguishing characteristic of a pebble ridge, washed by the sea on one side and by the waters of the Fleet estuary on the other. From thence it proceeds to the Ferry bridge, where it meets the waters of Portland Roads (from which, however, it is separated by a stretch of sand of varying width), and from thence to Portland.

Its direction after leaving Abbotsbury is W.N.W. and E.S.E. very nearly. On reaching Portland it takes a sharp curve to the west and forms the little bight called Chesil Cove, and it is here that the ridge begins to decline in height, and the pebbles, that up to this point have been gradually increasing in size, commence to diminish in bulk. A line stretched seawards from this point at right angles with the shore would point W.S.W.

The decline is rapid, so that in a distance of about 250 yards the bank tails out to nothing at the point where it touches, and does but just touch, the Undercliff.

There are probably several causes at work in bringing about this abrupt termination of the Chesil Bank. Among them I should reckon as most effective the curvature of the bank at Chesil Cove, whereby the beach is exposed at such an angle to the waves caused by the prevailing S.W. wind that the progressive action of the W. and W.N.W. winds is neutralised; secondly, the peculiar set of the tides round the Bill at Portland; and thirdly, the progressive action of the W. and W.N.W. winds being diminished or nullified by the curvature.

There cannot be the slightest doubt that the march of the pebbles is from Bridport to Portland, and that any movement in the contrary direction is due to temporary causes only.

That the larger pebbles travel over the heads of the smaller when the waves strike the beach at an angle is not merely probable in theory, but a fact demonstrable by experiment, as was announced by Sir John Coode in his elaborate paper on "Sea-Beaches" (Phil. Trans. 1834).

As to the materials of the beach having been partly derived from the destruction of the ancient raised beach, the remains of which are to be seen at this day in Portland, I would remark that, according to the account given by Leland in his "Itinerary," Portland at the time of his visit was of nearly the same dimensions as now, though tradition reports that the site of the old church was once the centre of the island, the shifting bank of sand and shells called the Shambles being its eastern boundary. Any pebbles derived from the intervening raised beach have in all probability been ground by the continual pounding of the Atlantic billows into sand long before this—probably before the time of Leland. Yet he states, with reference to the Chesil Bank, "that as often the wind blowith strene at south-est (? west) so often the se betith it, and losith the bank, *and breakith through it;*" indicating that the bank was not so strong then as it is now: for such a thing has not occurred within the memory of living man, not even on the occasion of the "Outrage" in Nov. 1823, when the crown of the bank was swept off by a tremendous gale, and spread over the sands on the other side of the ridge; when the fishermen's houses, that for centuries probably had nestled